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CONTENTS

THE AIMS AND BOUNDARIES OF PHYSIOLOGY[1]

PHYSIOLOGY, as the passing generation has known it, took shape and established its boundaries in this country just fifty years ago, when, shaking off its long subordination to anatomy, it was brought to a new life of recognition and progress. The seventeenth century had seen England famous for her school of physiologists, leading the rest of the continent in experimental results and in new ideas. Working upon the foundations laid by Harvey, that brilliant group at Oxford—Boyle, Lower, Mayow, Willis—had brought new light to the study of the living body. Nor was their service only recognized by fellow-workers abroad or by those that came after. Their names and fame were on fashionable lips; like that of their predecessor Harvey himself, under Charles I, and of that other Cambridge philosopher Glisson, their immediate contemporary, their work was aided by the direct interest and favor of the sovereign. But, during the eighteenth century and the earlier part of the nineteenth, eclipse fell upon the light that had thus burned so brightly, though isolated gleams shone here and there. James Jurin, under George II, applied the Newtonian principles to calculating the work done by the heart and to other problems of the body, but his efforts to lay true and exact foundations for the study of disease were premature in the absence of experimental data. Stephen Hales, chaplain to the future George III, made the first measurements of blood pressure in his garden at Teddington, and made many far-reaching observations of the first importance; but, as he wrote, there was indeed "abundant room for many heads and hands to be employed in the work,

[1] From the address of the president of the Section of Physiology at the Edinburgh Meeting of the British Association for the Advancement of Science.

for the wonderful and secret operations of Nature are so involved and intricate, so far out of the reach of our senses . . ."; and it was not then or till much later that many heads and hands were ready to be employed. Neither of these men had effective influence upon the thought or practical affairs of their day, either within the universities or outside them.

Physiology, as we know it now in this country, took its shape in a new revival which may be reckoned as beginning half a century ago. All our chief schools may be said to derive their lineage from that new home of active and unshackled inquiry—I mean University College, in Gower Street, London—and from the influence there of an Edinburgh graduate, William Sharpey, who at the age of thirty-four was taken from the Edinburgh school to be professor of anatomy and physiology. Here, from 1836 to 1874, Sharpey was inspiring a group of younger minds with his eager outlook. Already in France the new experimental study of the living functions was being established by Claude Bernard—that true "father in our common science," as Foster later called him; already in Leipzig Ludwig, transmitting the impulse of Müller's earlier labors, had founded that school of physiology which moulded the development of the subject in Germany and other countries, and had very strong early influence upon several of those who were later to become leaders with us. England had lost the pre-eminence that Stuart kings at all events had valued and promoted. Learning had become identified in English society with the mimetic use of the dead languages, and progress at the two universities—even at the Cambridge of Newton, where mathematics kept independence of thought alive—was still impeded by the grip of ecclesiastical tradition and by sectarian privilege. But at University College learning had been unfettered. Here Sharpey and his colleagues were in touch with the best progress in France and Germany, and here the organized study of physiology as a true branch of university study may be said to have begun. Its formal separation from anatomy came later and irregularly; a separate chair of physiology was not created at University College until 1874, nor at Cambridge or at Oxford until 1883.

We ought in piety to recognize that this tardy reflection of continental progress in our own subject, like parallel movements in other subjects, had in its early stages received invaluable aid from the Prince Consort, who, familiar with the progress of other countries, had lent his influence and sympathy to many men of science in their struggle against the insularity and apathy of the wealthy and governing classes of the earlier Victorian days. The curious may take note that the first outward mark of recognition given by the official and influential world to the existence of physiology as such was given not, as in other and poorer countries much earlier, by the endowment of some chair or institute for research and teaching, but by an act of symbolic representation. For, when the expensive statuary of the Albert Memorial was completed in 1871, it was found that "Physiology," betokened by a female figure with a microscope, had been given its place among the primary divisions of learning and investigation acknowledged in that monument to the Prince.

From Sharpey himself and his personal influence we may trace directly onwards the development of all the chief British schools of physiology whose achievements have in the past half-century restored Britain to more than her old pride of place in this form of service to mankind. We here fittingly acknowledge first the close link with Sharpey which we find here to-day in Sir Edward Sharpey Schafer, who, after fruitful years in his old teacher's place at University College, brought that personal tradition back to this great school of Edinburgh from whence it originally came. At University College itself the line has been continued with undimmed lustre by Starling and Bayliss and their colleagues to the present day. From Sharpey's school again are derived the great branches which have sprung from it, both at Oxford and at Cambridge. Burdon Sanderson, Sharpey's immediate successor at University College, proceeded thence to Oxford and founded there, against many difficulties of prejudice and custom, the school of physiology which Gotch, Haldane, and Sherrington have nevertheless maintained so brilliantly in succeeding years. To Cambridge, Michael Foster, one of Sharpey's demonstrators, was invited

in 1870 by Trinity College to be prælector in physiology and fellow of the college. This enlightened and then almost unprecedented act, no less than the personal qualities of Foster that so aboundingly justified it, I would, as in private duty bound, hold here in special remembrance. Under Foster's influence there came into being at Cambridge a strong and rapidly growing school of physiologists, from Langley, Gaskell, Sherrington, Hopkins, to numerous successors. There sprang from him, too, a new impetus to other subjects, through his pupils Francis Balfour and Adam Sedgwick to embryology and zoology, through Vines and Francis Darwin to botany, through Roy to pathology. From Foster again through Newell Martin, who, coming with him from London, had caught not only inspiration from him but some of his power of inspiring others, and who left Cambridge for a chair at Baltimore in 1876, we may derive a large part of the growth and direction of physiology since that time in the United States and in Canada. The rapid progress of all these biological sciences at Cambridge within a single generation, and the volume of original work poured forth depended, of course, upon two necessary conditions. The first is one which has never failed in this country—the existence of men fitted by temperament to advance knowledge by experiment. The second has been the supply of living necessities through the ancient endowments of the colleges, and these in the Cambridge of the last half-century have been freely and increasingly used in catholic spirit for the increase of any of the borders of knowledge.

If these have been the chief lines of descent along which our present heritage has come to us, as mind has influenced mind and the light has been passed from hand to hand, what has been the outcome as we look back over the half-century to those small beginnings?

Truly we can say that the workers in this country have in that short space of years laid the whole world under a heavy debt. In whatever direction we look we seem to see that in nearly all the great primary fields of psysiological knowledge the root ideas from which further growth is now springing are in great part British in origin, and based upon the work of British experimenters. If we consider the blood circulation we find that our essential ideas of the nature of the heart-beat were established by Gaskell, and that other first principles of its dynamics and of its regulation have been laid down by successors to him still with us; that the intricate nervous regulation of the arterial system has had its chief analyses here, and that here have been made more recently the first demonstrations of the part played by the minute capillary vessels in the regulation of the distribution and composition of the blood. Of the central nervous system the modern conceptions of function in terms of the purposive integration of diverse impulses along determined paths have sprung direct from British work, while the elementary analysis of the structure and functions of the sympathetic nervous system has been almost wholly British in idea and in detail. As with the nervous regulation of the body, so with the chemical regulation of function by traveling substances —the so-called "hormones," or stimulants from organ to organ—this, too, is a British conception enriched by numerous examples drawn from experimental work in this country. In the study of nutrition, of the primary "foodstuffs," proteins, carbohydrates, fats, salts and water, whose names in their supposedly secure sufficiency were written with his own hand by Foster upon the blackboard shown in his portrait by Mr. John Collier, to typify, as we may imagine, a basal physiological truth, we have come to learn that these alone are not sufficient for growth and life in the absence of minimal amounts of accessory unknown and unstable substances, the so-called "vitamins," which are derived from pre-existent living matter. This conception, undreamt of to the end of the nineteenth century, has fundamental value in medicine and in agriculture, and has already begun to bear a harvest of practical fruit of which the end can not be seen or the beneficence measured. This discovery stands to our national credit, and large parts of its development and application have been due to recent British work. If we turn to the regulation of respiration and its close adaptation to body needs, that also, as it is now known to the world, is known as British labors have revealed

it, just as the finer analyses of the exchanges of gas between the air and the blood and between the blood and the body substance have been made with us. The actual modes by which oxygen is used by the tissues of the body, its special relations to muscular contraction, the chemical results of that contraction, the thermal laws which it obeys—all these fundamental problems of living matter have seen the most significant steps to their solution taken within the past generation in this country.

Work of this kind brings permanent enrichment to the intellectual life of mankind by giving new and fuller conceptions of the nature of the living organism. That we may think is its highest function; but it does more than this. Just as all gains in the knowledge of Nature bring increase of power, so these discoveries of the past fifty years have their place in the fixed foundations upon which alone the science and the arts of medicine now or in the future can be securely based. The special study of disease, its cure and prevention, has had notable triumphs here and elsewhere in the same half-century, and these as they come must make as a rule a more spectacular appeal to the onlooker. Yet it is the accumulating knowledge of the basal laws of life and of the living organism to which alone we can look for the sure establishment either of the study of disease or of the applied sciences of medicine. As we have seen, there are few indeed among the fields of inquiry in the whole range of physiology in which the British contributions to the common stock of ascertained knowledge or of fertile idea do not take a foremost place. It would be impiety not to honor, as it would be stupidity to ignore, these plain facts, which, indeed, are now perhaps more commonly admitted abroad than recognized at home. There is no occasion here for any spirit of national complacency—rather the reverse, indeed. British workers at no time earlier than the war have had the menial assistance or other resources which their colleagues in other countries have commonly commanded, and too often the secondary and relatively easy developments of pioneer work done in this country have fallen to well-equipped and well-served workers elsewhere. If in the past half-century better support had been available from public or private sources, or at the older universities from college endowments, it is impossible for any well-informed person to doubt that a more extended, if not a more diversified, harvest would have been won.

We stand too near to this remarkable epoch of progress to appraise it fairly. In the same span of years Nature has yielded many fresh secrets in the physical world under cross-examination by new devices which have themselves been lately won by patient waiting upon her. So great a revelation of physical truth has been lately made in this country, bringing conceptions of space and of matter so swiftly changing and extending, that our eyes are easily dimmed to the wonders of that other new world being unfolded to us in the exploration of the living organism. Only the lapse of time can resolve the true values of this or that direction of inquiry, if indeed there be any true calculus of "value" here at all. We seem to see in the progress of physiology, not at few but at many points, that we stand upon new paths just opening before us, which must certainly—as it seems—lead quickly to new light, to fuller vision, and to other paths beyond. The advances of the next half-century to come must far exceed and outshine those due to the efforts of the half-century just closing; that is probably the personal conviction of us all. Yet we may still believe that through all the history of mankind recognition will be given and honor be paid to the steps in knowledge which were made first and made securely in the period we now review. The men who have done this work will not take pride in it for themselves; they know that their strength has not been their own, but that of the beauty which attracted them, and of the discipline which they obeyed. They count themselves happy to have found their favored path. Other and more acute minds might have usurped their places and found greater happiness for themselves if, under a social ordering of another kind, they had been turned to the increase of knowledge instead of to the ephemeral, barren, or insoluble problems of convention and competition. By how much the realized progress towards truth and the power brought by truth might have been increased under a changed

social organization we can never know, nor can we guess what acceleration the future may bring to it if more of the best minds are set free within the state for work of this highest kind, what riches may be added to intellectual life, or what fuller service may be given to the practical affairs of man and to the merciful work of medicine.

WALTER FLETCHER

TETRACHROMATIC VISION AND THE DEVELOPMENT THEORY OF COLOR

IT would seem to be time for the poor children in the kindergarten to be taught that the number of different "colors" in the spectrum (and in the whole world of natural objects as well) is not seven, nor six, but simply four —red, yellow, green and blue. We have been told lately by Dr. Jennings, in the *American Journal of Physiological Optics,* that the number is seven, and by U. S. Public Health Bulletin No. 92 (prepared by direction of the Surgeon General) that the number is six. The Milton Bradley Company, which furnishes countless delightful kindergarten objects for the children, follows the customary delusion that there are six. But every psychologist knows by this time, thanks to the life-long labors of Hering, that the number of different chromatic sensations (chromata) furnished by the spectrum, and by all of nature too, is four. No physicist, however, is as yet aware that there are more than three; I am in the habit of saying that the physicists are all psychically blind to both yellow and white, all save one, Professor Robert Wood, who in his *Physical Optics* explicity recognizes the existence of a "subjective" yellow. In course of time, no doubt, even the physicists will recognize the fact that *all* the color sensations are "subjective"—that there are no reds, greens, etc., in the extra-corporeal world, but that there are simply the erythrogenic, xanthogenic, chlorogenic and cyanogenic light rays—and that any ray-combination that looks white (as, for instance, a mixture of "yellow" and "blue" light) is a leucogenic combination and due to a "leuco-base."

The reason that led Newton to find seven colors in the spectrum was an æsthetic one—the spectrum is, counted in wave-lengths, about an octave long; in the music octave we recognize seven notes, so why not assign seven tones also to the color octave? In this way what was common knowledge in regard to the number of colors in the world from the time of Leonardo da Vinci became vitiated for a hundred and fifty years by an error which it is still hard to recover from. Hering, in opposition to Helmholtz, recognized that there are four chromatic sensations, but he too was led astray by a logico-aesthetic consideration; he thought it would be nice if, since red and green are, like blue and yellow, a "disappearing" color pair, they were also a white-constitutive color pair. So he said we will assume that they *are* a white-constitutive color pair, and to make the situation still more pleasing we will assume that black and white too are at least a disappearing color pair. But I have shown that when you take the exact red and green (or, in fact, anything near them) you get, on mixing, not white but yellow. My contention on this point has been accepted by Westphal, by v. Kries and others; the colors which are complementary, or white-constitutive, are, as Titchener, with a degree of honesty which is unusual in the followers of Hering, admits, not red and green, but crimson and verdigris,—in other words, white is here, as elsewhere, made out of red, green and *blue.*

Normal, mid-retinal, vision is tetrachromatic. It is to be hoped that we may sometime persuade the Milton Bradley people (whose red, green, yellow and blue papers are, as I have shown, very near to the exact, unitary, Red, Green, Yellow and Blue—I write these color-names with capitals when the colors are exact), and the United States government as well, that the *different* colors in the spectrum are four in number, and that if one adds to one's papers two of the dual color-blends, red-blue and red-yellow (the so-called purple and orange), one should add also the remaining dual color-blends, green-blue and green-yellow. (The fact that these last two color-blends have no misleading unitary names is so much to the good). At-

tention should be called at the same time to the curious fact that though you may easily have the *physical* conditions (the proper light-ray mixtures) for the two other possible dual color-blends (the red-greens and the yellow-blues), these are *sensations* that never occur—their places are taken, respectively, by yellow and by white.

I should like to mention that the color theory which I have proposed (the development color theory) is the only one in existence which holds together (the function of a theory), and makes reasonable, the three fundamental color-sensation facts (and the other, subsidiary, facts as well). These are:

A. The Helmholtz fact: the basis of color-vision is a *three-receptor* (chemical) process, —the "red," "green" and "blue" light-rays *are sufficient* (on mixing) to reproduce the whole gamut of the color sensations.

B. *Nevertheless* (the Hering fact) yellow and white are also unitary sensations and not any sort of sensational color blends, although they may be produced by physical light-ray mixtures. Hering thus corrects what I have called the psychical color-blindness (to yellow and to white) of the Helmholtz school, but at the cost of concealing from his followers all the facts which are mapped out in the Helmholtz triangle, or what the metallographers call when they make a diagram of their ternary alloys—a less frightening word perhaps—the "tri-axial diagram." The color-triangle, in other words, is nothing more than the representation of mixed color-constitution in terms of trilinear coordinates; what could be more natural when the variables which are both sufficient and indispensable are three in number?

C. Of equal importance is the fact of the order of phylogenetic development of the light-sensations (achromatic and chromatic). It is, in its three successive stages, as has been perfectly well made out, this:

(1) A white-sense only, achromatic vision (furnished by the more primitive retinal elements, the rods), which occurs (*a*) in the lower animals, such as lived, for instance, in carboniferous times (when colored flowers and colored birds did not yet exist)[1], (*b*) in those defective individuals who have achromatic vision only, and (*c*) in the far periphery of our own retina.

(2) Dichromatic vision—the spectrum is yellow at one end, blue at the other (but in place of what should be the yellow-blues appears white). This is the vision (*a*) of the bees (v. Frisch), (*b*) of the partially color-blind, and (*c*) of our own mid-periphery.

(3) Complete, tetrachromatic, color-vision, —the red and green sensations have been added; but where we have the *physical* conditions for seeing the red-greens, or the red-green-blues, *yellow* and *white*, respectively, take their places.

The theory of Helmholtz is (as Professor Cattell has well said) both pre-psychological and pre-evolutionary. That of Hering (besides being otherwise impossible) is pre-evolutionary: there is no question that red and green (which *revert* to yellow) are *developed out of* yellow. But worse than this—each of these theories is utterly contradictory to the facts which the other theory is expressly built up upon. This circumstance has not hitherto been sufficiently noticed; this Mr. Troland says ("The Enigma of Color Vision," American Optical Society, p. 8): "The Young-Helmholtz theory is preferred by physicists because it lays emphasis primarily upon the stimuli to vision, while the Hering theory receives more attention at the hands of the psychologists because its fundamental conceptions are derived from introspective analysis." This is true, but it is very far from being an adequate account of the situation. (1) The Young-Helmholtz school not only assumes but *proves* (not, as is often said, by means of the König-Dieterici spectral distribution curves by themselves, but by the complete coincidence of these curves with those, respectively, of the

[1] We have no means of knowing whether our own background sensation, the non-light sensation, that of blackness (which exists for the purpose of filling up our visual field), came in with the first, non-specific, light-sensations, or only later. There is some ground for thinking it arose later. I discuss this question in my coming article on "The Sensation of Blackness."

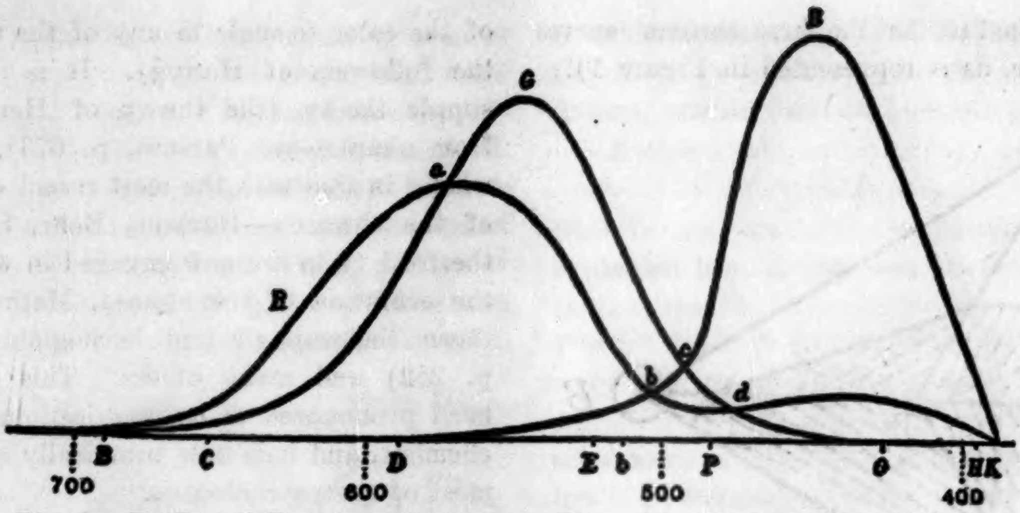

Fig. 1. *R*, *G*, and *B*, resonance curves. These are the curves of König and Dieterici corrected to new determinations of the points of section, *a*, *b*, *c*, *d*. Abscissæ, wave-lengths of the interference spectrum of the arc light; ordinates, arbitrary scale. (F. Exner.)

two types of yellow-and-blue vision)[2] that the number of "primary" colors is *three* and not four. (Fig. 1). This is fact. (2) The Hering school has only to ask for the most cursory examination of the gamut of color-sensations to show that the number of its different chromatic constituents is four, not three. This is fact. But the group of facts subsumed under (1)—the facts of "matching by mixture" (Fig. 2)—is absolutely incompatible with the theory of Hering,[3] and the group of facts subsumed under (2) is absolutely incompatible with the theory of Helmholtz. It is little to the credit of any association of scientists (for instance, the Optical Society of America) that they still solemnly discuss the theories of Helmholtz and of Hering. The situation is simple: each of these theories is absolutely refuted by the *facts* which are the groundwork of the other.

I have devised a simple diagram by means of which one can keep in mind the impossibility at once of the Helmholtz and of the Hering theory. Color diagrams are immensely more illuminating if they are done up in color.[4] But lacking that one can make shift with appropriately striated surfaces. I call this diagram my Quadrigeminal Color Body (a term suggested by the *corpora quadrigemina*), but it is at the same time triaxial. (The triangle should always be drawn with the YB line a horizontal (fundamental) line, as indicative of the fact that yellow and blue were developed first—that red and green were a later addition. (The actual spectral line approaches nearer to the point W in the green region on

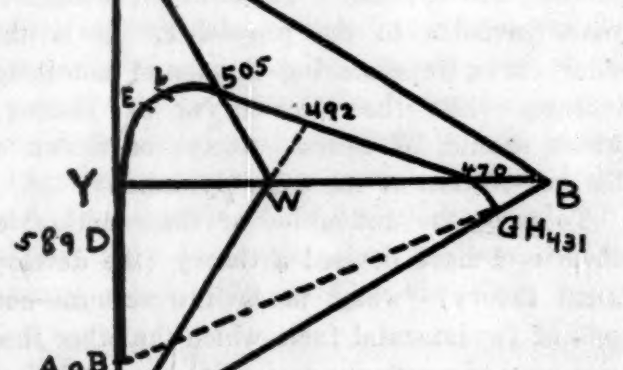

FIG. 2. The color triangle, which exhibits "matching by mixture."

[2] *Dictionary of Philosophy and Psychology*, Art Vision, II, 788.

[3] The attempt of v. Kries to supplement the Helmholtz theory by supposing that the three colors resolve themselves into four at a higher level of the visual nerve system is a purely *ad hoc* hypotheses, and without significance. See my articles on "The Theory of Color Theories," *Comptes rendus du V/e Congrès intern. de Psychologie*, Genève, 1909, and *Psychological Review*, May, 1922.

[4] Stoelting is putting on the market for me my complete set of colored color diagrams.

account of the fact that the three chroma[5] curves overlap here, as is represented in Figure 1).

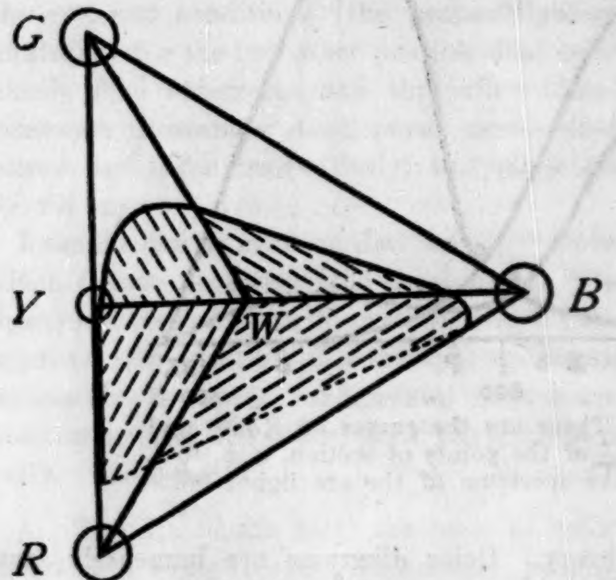

FIG. 3. The *quadrigeminal* color area—*triangular* in shape.

This figure illustrates the fact that while the color field is a function of three variables when you reproduce it by the mixing of specific lights, you no sooner look at it than you see that it consists of *four* distinct regions—the whitish yellow greens, the reddish bluish whites, etc. It suffices to upset at once the two "antiquated" (as they call them at the University of Chicago) current theories. I make the yellow circle small to indicate (*a*) that yellow occupies a very narrow region in the spectrum, (*b*) that it is the result of a secondary retinal process, and (*c*) that it has been for a hundred years invisible to the physicists. It is this color curve, representing facts and not imaginations (like the color-curves of Hering), which should, of course, always be drawn as the belt-section of the color-pyramid.

To meet the difficulties of these antiquated theories I have devised a theory (the development theory) "which takes into account both sets of fundamental facts which the other theories were respectively devised to explain," facts which do however in reality collectively refute them both. (You will not find even a picture of the color triangle in any of the writings of the followers of Hering). It is a perfectly simple theory (the theory of Hering is far from simple—see Parsons, p. 673), and it is wholly in line with the most recent conceptions of the chemists—Harkins, Bohr, Soddy, Rutherford (who are now engaged in working out the evolution of the atoms), Mathews, Willstäuer (chlorophyll and hæmoglobin, Bayliss, p. 252) and many others. This theory has been pronounced to be unobjectionable by the chemists, and it is now practically accepted by most of the psychologists.

The theory in brief is this:

There is probably no other organ in the body in which the record of development has been preserved in such a remarkable fashion as in the organ of vision. We have, *pari passu* with the successive stages of specificity of response to the visual spectrum, represented in Fig. 3,

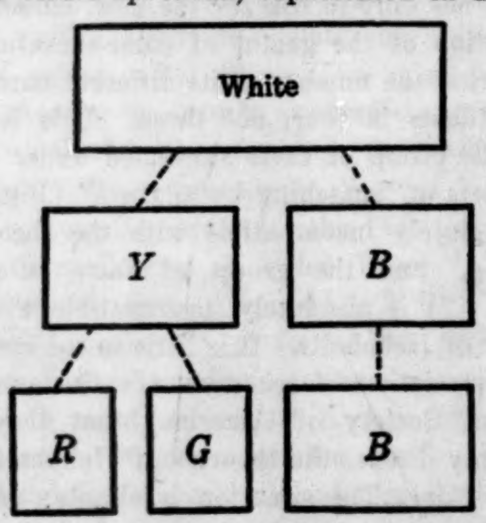

FIG. 4. Stages 1, 2 and 3 of the actual development of the color sense.

(1) an anatomical development of rods into cones, and (2) a chemical development of the rod-pigment sensitizer such that in man only there is an intermediate stage, the visual yellow, between the "visual purple" and the final leuco-base (König, Garten). What more natural than to suppose that there has been also a development of the light-sensitive receptor substance in the receptor organs (rods and cones) of the retina? This developing substance must, however, be at the same time of such a nature as to account for the singular fact (unknown in any other region of sense) that the colors successively developed are dis-

[5] I have been constantly urging, since 1913, the use of the word *chroma* (plural, *chromata*) to obviate the shocking ambiguity in the present meaning of "color."

appearing color pairs—they produce a more primitive white, or yellow (see above). If these facts are held distinctly in mind, the appropriate chemical conception almost forms itself. I represent, purely diagrammatically, of course (Burdon-Sanderson[6] especially noted this point when my theory first came out) that portion of a molecule which is capable of being dissociated out of light in the way indicated in Figure 4.

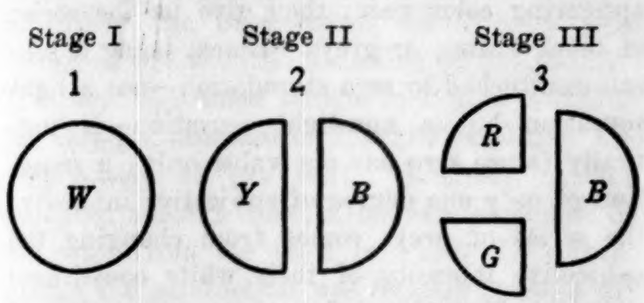

FIG. 5. The cleavage products in the three stages of the color sense. This diagram does not represent the entire light-sensitive molecule, but only the specific cleavage products which, according to the Ladd-Franklin theory, constitute the several nerve excitants for the color sensations (From Woodworth's *Psychology*). For other diagrams, see *Psychological Review*, 23: 247, 1916; *Zeitschrift f. Psychologie*, Bd. 6, etc.

The development required is that of a greater and greater *specificity* to the electro-magnetic vibrations of the visible spectrum. A portion of a molecule which at first is broken off indifferently by the whole spectrum becomes in a second stage more specific,—by a fresh aggregation of atoms a portion *Y* responds to the yellow end of the spectrum, a portion *B* to the blue end of the spectrum. But what happens when yellow and blue light fall at once on this chemical substance? The *Y* and the *B* (since they are the chemical constituents of *W*, because the assumption is that they were segregated out of it) will chemically unite and will produce *W*, the nerve-excitant of the sensation white. In the same way in the third, and latest, stage, the newly segregated *R* and *G*, when torn off from the molecule by light of low and of high middle frequency, will revert to the mother substance *Y*; and if light of high frequency, "blue," is now added, we shall again have the nerve-excitant of white. That is to say, just as when we have in a test-tube the chemical constituents of HCl (namely, H and Cl) they chemically unite, under proper conditions, and produce HCl, so in a cone we have

$$R + G = Y$$
$$Y + B \; (= R + G + B) = W$$

Observe that we have now, quite incidentally, explained how it happens that lights of only *three* specific, homogeneous wave-lengths ("red," "green" and "blue") are sufficient to reproduce the whole gamut of color sensations, including yellow and white, which the psysicists have never noticed the existence of.[7] Yellow is a secondary product, and so is white, but they are both perfectly good unitary sensations. The theory explains at the same time, of course, how it is that the primitive white mediated by the rods is the same sensation as the white made out of (in the highly developed cones) yellow and blue, or red and green and blue. There is no reason, of course, as Professor Carr has pointed out to me, why there should not be also some of the more primitive chemical substances in the cones of the central retina.

Since the interesting work of Hecht, it seems to be quite certain that the first effect of light on the retina is photo-chemical (which is, of course, the same thing as electrical). It is here, without question, that is found that "transformer mechanism," as I have called it, by which what should look to us like 165 *different* colors in the spectrum is replaced by a paltry four—the best that Nature could accomplish with only one small cone to work in. Five unitary colors (including white) and mixtures of them—the color blends—are all that we can see in the 30,000 *discriminable* sensations that are given us by light. It is nerve impulses produced by retinal chemical stimuli of the character which I have described (or of some other character) that mediate the processes which take part in the final "neuro-

[6] Presidential Address, British Association, *Nature*, Vol. 48, p. 469.

[7] Crowther, a prominent English physicist, actually says that the white produced by mixing homogeneous yellow and blue light-rays is not a "real" white—that, he thinks, *must* be a whole-spectrum white. Professor Titchener, on the other hand, has called my attention to the fact that on my view white is *always* (aside from the most primitive stage) due to a union of yellow and blue constituents.

psychic correlation"—a term which I have proposed as much preferable to psychophysical parallelism—in the domain of color.

In a recent discussion of my color theory (*Am. J. Physiol. Optics*, 1920-21) it is maintained that it would be more "advanced" to regard as of "prime importance" the cortical processes: "It appears to me that the Ladd-Franklin theory postulates the existence in the retina of conditions of sensation of the sort required for the processes in the cerebral cortex which directly underlie the visual consciousness, but which are not required and probably do not exist in the case of the retina." In reply to this it is only necessary to point out that if a "mechanism of defect" (such as would result in loss of consciousness for the red-greens and the yellow-blues) is found to occur in any part of the light-sensation chain (rods and cones, bipolar cells, third neurons, corpora quadrigemina, optic thalamus, cortex) that defect cannot be recovered from in any one of the later stations of the nerve impulse—if R and G have once reverted into Y, and Y and B into W, anywhere in the visual circuit, it is not necessary to provide for their doing it again in the cortex; on the other hand, if this defect has *not* happened lower down—if a separate "blue" and "yellow" have successfully reached the cortex—it is improbable that Nature, out of pure *Bösartigkeit*, should have introduced in the cortex a mechanism for their extinction.[8]

v. Kries has objected that in my theory it is not explained how the same sensation of whiteness should be mediated directly in the rods and in the more highly developed cones out of a physical mixture of red, green, and blue lights, but, as I have pointed out above, this is exactly what my theory does explain.

It has been called to my attention that several of the psychologists, while practically adopting my theory of color sensation, express the opinion that I have given no explanation of the sensation of black. But that is not the case; I have not, it is true, discussed black very frequently, and that, I believe, for two reasons. (1) It is very simple—it has no connections with any other of the color-sensations. The reason that color "theory" is so important, and has been so contended over, is that the facts of color (excluding black) are so very mysterious: why do we fail to see the yellow-blues and the red-greens, and why do we get, respectively, *white* and *yellow* in their place? Blackness stands by itself—it has no such queer relations with any of the other colors. Black and white, for instance, are *not* a disappearing color pair; they give us the series of black-whites, or greys. Black, being a sensation attached to zero stimulation—not a light sensation but a non-light sensation—is naturally (since zero has one value only) a sensation of only one degree of subjective intensity; the series of greys comes from changing the subjective intensity of their white constituent only. A blue-green of a given proportion of blueness and greenness we can see in dozens of different intensities; not so a grey. Give a certain grey a higher illumination and you change the quality as well as the brightness of your black-white blend. Professor G. E. Müller has dwelt upon this latter fact, but he has given a wrong interpretation of it; Wundt also, although his theory of color is negligible (and has been neglected), puts this situation correctly. It is easily accounted for on my theory.

A fuller account of the Development Theory of Color Sensation will be found in most of the recent books on psychology,—as Calkins, Judd, Angell, Breese, Watson, Warren and Woodworth. I have discussed it also in the *Psychological Review*, 23, 1916, and 29, 1922; in the *Am. Cyclop. of Ophthalmology*, 1913; in the *Dictionary of Philosophy and Psychology;* in *Mind*, 1892, 1893, and in SCIENCE, 22, pp. 18-19. In the last two places I have discussed its fundamental difference from the theory of Donders. My theory has been taken over by Schenck without due acknowledgment, as has been pointed out for me by v. Brücke (*Zentrlbl. f. Physiologie*, 1905). It has suffered from not having been criticised enough; some criticism of it by v. Kries and by Troland I have discussed very fully in *Practical Logic and Color Theories* (*Psychological Review*, May, 1922).

CHRISTINE LADD-FRANKLIN
COLUMBIA UNIVERSITY

[8] I discuss this point more fully in the *Psychological Review*, May, 1922.

SCIENTIFIC EVENTS

STATISTICS OF THE ALASKA FISHERIES FOR 1921

Statistics of the Alaska fisheries for 1921 have recently been completed and are summarized as follows: The total active investment in the fisheries was $39,001,874, a decrease of $31,984,347 from 1920. The industry gave employment to 15,070 persons, or 12,412 less than in 1920. The products of the fisheries were valued at $24,086,867, a decline of $17,405,257. The pack of canned salmon in 1921 was 2,596,826 cases, a decrease of 1,832,637 cases, or approximately 41 per cent. Southeast Alaska produced 803,071 cases, a decrease of almost 65 per cent. from the pack in 1920. In central Alaska the production was 643,099 cases, a decrease of almost 52 per cent. In western Alaska the pack was 1,150,656, an increase over 1920 of 283,652 cases, or over 32 per cent. The total value of canned salmon was $19,632,744. Other products of the salmon fisheries were mild-cured, pickled, fresh, frozen, and dried and smoked salmon, which had an aggregate value of $1,335,818. Salmon by-products, consisting of oil and fertilizer, were valued at $18,022. The total catch of salmon in Alaska in 1921 was 37,905,591 fish, as compared with 65,080,539 in 1920, a decrease of approximately 41 per cent.

The number of salmon canneries operated in 1921 was 83, or 63 less than in 1920. Of this number the southeastern district was credited with 30 (decrease of 52), the central district with 25 (decrease of 11), and western Alaska with 28 (the same as in 1920). Comparisons of figures as to gear used are as follows: 180 traps, of which 127 were driven and 53 floating, were used in 1921, a decrease of 318 driven and 155 floating traps from 1920. Seins decreased from 712 to 213, representing a reduction of 82,048 fathoms of webbing. The total length of gill nets was 375,320 fathoms, a decrease of 85,627 from 1920.

Values of products of the other fisheries were as follows: Halibut, $1,476,450; herring, $934,044; cod, $457,320; shrimps, $132,077; crabs, $33,180; whales, $19,950; trout, $18,925; sablefish, $17,985; clams, $9,940; red rockfish, $362; and smelts, $50.

FELLOWSHIPS OF THE NATIONAL RESEARCH COUNCIL

The National Research Council announces for the next academic year a number of fellowships for fundamental investigations on agricultural applications of sulphur. The funds for the fellowships have been provided by a grant from the Texas Gulf Sulphur Company.

These fellowships, each carrying an annual stipend of approximately $1,000, will be administered by a special sulphur fellowship committee of the advisory board of the American Society of Agronomy, in conference with the executive committee of the division of biology and agriculture of the National Research Council. Inquiries and appplications should be addressed to the Sulphur Fellowship Committee, National Research Council, Washington, D. C.

It is proposed that the work to be prosecuted under these fellowships will include investigations on the value of sulphur in the control of potato scab, nematodes, soil insects and sweet potato disease; also the value of sulphur as a fertilizer for alfalfa and other legumes and the effect of sulphur on alkali soils.

Applicants for the fellowships must be graduate students in universities and colleges or competent members of experiment station staffs. Fellows are expected to devote practically their entire time to the investigations, excepting only such course work as may be necessary to meet the requirements for an advanced degree. While no definite assurance can be given, it is expected that support for the fellowships will be extended from year to year for a period as the results may warrant.

In order to prevent possible confusion, it is pointed out that these fellowships are entirely distinct from the two sulphur fellowships recently announced (Science, March 24) by the Crop Protection Institute and administered by it in cooperation with the National Research Council.

REVIEW OF APPLIED MYCOLOGY

The Imperial Bureau of Mycology has undertaken the publication of a monthly abstracting journal, the *Review of Applied Mycology,* for the purpose of supplying, month by month, a summary of the work published

in all countries on the diseases of plants and various other aspects of economic mycology. The first number was issued in January, and it is hoped to complete a volume of between four and five hundred pages annually. The announcement says:

Mycologists and plant pathologists often find it difficult to keep themselves informed of the progress of work in other countries. The publications in which an account of current work is given are very numerous and are scattered through a large number of journals, many of which only occasionally contain an article of interest. There are few, if any, libraries in which all these publications can be found, while the working mycologists in the overseas part of the British Empire often have access to only a small proportion of them. The committee of the Imperial Bureau of Mycology has accordingly felt that it is desirable to start the publication of a compact yet comprehensive survey of current literature dealing with the various aspects of applied mycology, on the lines of the *Review of Applied Entomology* published by the Imperial Bureau of Entomology in London. While *Botanical Abstracts* remains the only journal that aims at giving a complete citation of the literature in all branches of botanical science, the present *Review* will be specially directed to supplying to workers with restricted library facilities, sufficiently full abstracts of papers on the diseases of tropical crops and other similar matters of interest to mycologists in the overseas parts of the British Empire to enable them to keep informed of the progress of current work.

Though the chief object of the new journal is to give an up-to-date summary of work bearing on the practical application of the study of plant diseases to the reduction of the wastage due to such diseases in agriculture, the fundamental researches on which most progress in this direction is based have a wider appeal. The *Review* will enable all those who are interested in the progress of science to follow the development of one of its younger branches; the student of pure science will, it is hoped, find many side-lights on the wider problems on which he is engaged; while the practical grower will be able to learn the experience in other countries with improved methods for controlling plant diseases.

Subscriptions, orders and all communications respecting the publication should be sent to the editor, Imperial Bureau of Mycology, Kew, Surrey, England.

THE PUBLICATION OF SCIENTIFIC PAPERS

IN view of its general interest to contributors to scientific journals, we are permitted to print the following letter addressed by Professor Ross G. Harrison, of Yale University, managing editor of the *Journal of Experimental Zoology*, to its contributors:

Owing to the high cost of printing and the consequent large deficit incurred in the publication of its journals, the Wistar Institute has notified the editorial board of the *Journal of Experimental Zoology* that, unless financial support is forthcoming, it will not be possible to print during the present year more than two volumes or one thousands pages of the *Journal*, instead of the three volumes of five hundred pages each published in 1921. Since the war material for publication has been coming in at a rapidly increasing rate, so that there is now on hand more than sufficient to fill the two volumes to be issued this year. This means that, under present conditions, manuscripts now received can not appear much earlier than eighteen months from date. It is hoped that before long conditions in the printing trade will become more favorable or that some method of financing the deficit may be devised. In the meantime, the editorial board find it necessary to ask your cooperation in meeting the present difficulties. This can best be done by making papers as concise as possible, by using the simplest form of illustration—such as can be reproduced by zinc engraving, by omitting tables as far as is consistent with clearness, and by avoiding duplication in publication.

The editors do not wish to set any arbitrary limit to the length of papers that can be accepted; for some are concise at fifty pages and others verbose at five. A colored plate may be a necessity in some instances and a useless expense in others. It is felt, however, that almost every paper would be improved by judicious pruning, and the authors, as the best qualified persons to do this, are asked to undertake the task. It is scarcely to be expected that even the utmost self restraint on the part of contributors will entirely meet the exigencies of the situation, so that the editors will probably have to exercise their judgment as regards the space that can be allotted to each paper submitted. Nevertheless, if contributors are willing to undertake drastic measures themselves, it will frequently spare the editorial board the necessity of declining papers which, under other circumstances, they would like to

accept, and it will serve the greatest good to the greatest number by giving every one a fair share in the use of our present limited facilities for publication.

THE GRANTS FOR RESEARCH OF THE NATIONAL ACADEMY OF SCIENCES

PROGRESS has been reported as follows on grants made by the National Academy of Sciences:

BACHE FUND

The researches of Carl H. Eigenmann, for which grants 214 and 220 were made, have been published in the *Memoirs of the Museum of Comparative Zoology*, Vol. 43, Parts 1 and 2, and in the *Proceedings of the American Philosophical Society*, the *Journal of the Washington Academy of Sciences*, and the Indiana "University Studies." The research on fishes of the upper Amazon basin and Lake Titicaca is still in progress.

A preliminary paper on the work of H. W. Norris on cranial nerves of amia and lepidosteus will be published shortly in the *Proceedings* of the Iowa Academy of Science.

Star counts have been made by H. Nort, of Gouda, Holland, for more charts of the southern hemisphere. Additional counts have been made to find distance correction for the Franklin Adams charts. Formulæ have been derived to compute the equatorial coordinates of the fields counted from the declination and the R.A. of the center of the plate and the focal length of the telescope used. The limiting magnitude for ten additional charts of the northern hemisphere has been derived.

Preliminary results of the research of J. C. Jensen, grant No. 218, have been published in the *Proceedings* of the Nebraska Academy of Science for 1919.

Results of the research of H. G. Barbour, of McGill University, grant No. 219, have been published in the *Proceedings* for Experimental Biology and Medicine, 1920; *The Journal of Pharmacology and Experimental Therapeutics*, 1921; and *The American Journal of Physiology*, 1921.

Preliminary results of the research of T. H. Goodspeed, of the University of California, grant No. 224, have been published in the University of California Publications in Botany, Vol. 5.

SMITH FUND

There was issued in 1921 as a publication of the Leander McCormick Observatory of the University of the University of Virginia, "349 parabolic orbits of meteor streams and other results," by Charles P. Olivier, a discussion of 22,000 observations of meteors made by members of the American Meteor Society. It is a comprehensive report of results of an investigation which has been aided by several grants from the J. Lawrence Smith Fund at various times since 1913 to Professor S. A. Mitchell, director of the McCormick Observatory, under whose supervision the work has been done.

SCIENTIFIC NOTES AND NEWS

DR. E. A. DE SCHWEINITZ, professor of ophthalmology at the University of Pennsylvania, gave the presidential address at the opening session of the American Medical Association held at St. Louis on May 23.

DR. E. W. RICE, JR., has been elected honorary chairman of the board of directors of the General Electric Company. He will devote his time particularly to the supervision of the scientific, engineering and technical work of the company in this country and abroad.

DR. ROSS AIKEN GORTNER, professor of agricultural biochemistry at the University of Minnesota, has been elected to the office of national president of Phi Lambda Upsilon, the honorary chemical society. He succeeds Dr. Harold A. Fales, of Columbia University.

THE annual meeting of the Iron and Steel Institute, under the presidency of Mr. Francis Samuelson, was held on May 4 and 5 at the house of the institution. The Bessemer Medal was presented to Professor Kotaro Honda.

DR. MURK JANSEN, of Leyden, has received the Umberto I prize awarded every five years by the province of Bologna for the best work or discovery in orthopedics.

AT the recent annual meeting of the American Academy of Arts and Sciences the election of the following fellows and foreign honorary members was reported by the council: Class I. The Mathematical and Physical Sciences: Walter Sydney Adams, Pasadena; Gano Dunn, New York; Thomas Alva Edison, Orange, N. J.; Edwin Crawford Kemble, Cambridge; Richard Chase Tolman, Washington; Arthur Stanley Eddington, Cambridge, England. Class II.

The Natural and Physiological Sciences: Nathan Banks, Cambridge; Thorne Martin Carpenter, Boston; Stanley Cobb, Canton, Mass.; Joseph Lincoln Goodale, Boston; Robert Williamson Lovett, Boston; Alfred Clarence Redfield, Boston; Austin Flint Rogers, Palo Alto; William Henry Weston, Jr., Cambridge; Sir Thomas Clifford Allbutt, Cambridge, England; Emmanuel De Margerie, Strasbourg, France. Class III. The Moral and Political Sciences: Edward Channing, George La Piana, William McDougall, Arthur Kingsley Porter, Paul Joseph Sachs, Charles Henry Conrad Wright, all of Cambridge; Henri Pirenne, Ghent, Belgium.

At the recent annual meeting of the members of the Royal Institution, London, the report of the board of visitors was presented and showed that last year 57 new members were elected while 38 were lost by death. The total membership in July last was 826 against 831 in the same month of the previous year. The result of the ballot for new officers was as follows: President, The Duke of Northumberland; treasurer, Sir James Crichton-Browne; secretary, Colonel E. H. Grove-Hills.

Dr. Ludwik Silberstein, mathematical physicist at the Research Laboratory of the Eastman Kodak Company, has been appointed an associate editor of the Journal of the Optical Society of America.

Dr. Paul M. Giesy, formerly with the Calco Chemical Company, has become research chemist with E. R. Squibb & Sons at their Brooklyn, N. Y. plant.

A. W. Hickman retired on March 31 from the United States Bureau of Animal Industry after thirty-four years of service. For the last fifteen years he was chief of the Quarantine Division.

Dr. Donald D. Van Slyke, member of the Rockefeller Institute for Medical Research, has accepted an appointment as visiting professor in biological chemistry at the Peking Union Medical College for four months, beginning with the fall term of the next academic year. Dr. Harry R. Slack, of the Johns Hopkins Medical School, will be visiting professor in oto-laryngology at the college for the academic year 1922-23.

Dr. Ernest Fox Nichols, who has been in Honolulu, returned on May 15 to his work as director of pure science in the Nela Park Research Laboratory of the National Electric Lamp Works at Cleveland.

Dr. Ruth Marshall, professor of zoology in Rockford College, will make an extended trip to Alaska this summer, visiting the Atlin Lake region, Cordova and Katchikan. She will collect water mites in these regions. Miss Patsy Hughes Lupo, associate professor of botany, will sail from Seattle to Nome on June 1, and will spend the summer in the interior, studying and collecting algæ and fungi.

Dr. Eloise Gerry, microscopist in the office of wood technology at the Forest Products Laboratory, left on May 20 for a field trip through Georgia, Florida and Louisiana. It is her purpose to make experiments and investigations that will assist in developing better methods of obtaining turpentine and rosin from living pine trees. Miss Gerry will work in cooperation with Mr. Austin Cary, of the Washington Office of the Forest Service, and Mr. Lenthal Wyman, of the Southern Experiment Station, members of the Florida National Forest organization and local timber owners.

Dr. Ross Aiken Gortner, professor of agricultural biochemistry at the University of Minnesota and national president of the honorary chemical society, Phi Lambda Upsilon, recently lectured at the Armour Institute of Technology and the University of Wisconsin on "The Colloid Chemistry of Wheat and Flour," and at the University of Michigan, Ohio State University and Purdue University on "Vital Phenomena as Colloid Processes." Both lectures were given at the University of Illinois.

Dr. C. E. K. Mees, director of the Research Laboratories of the Eastman Kodak Company, gave a lecture entitled "A photographic research laboratory" before the Northeastern Section of the American Chemical Society on May 12.

Professor E. Mellanby delivered the Oliver Sharpey lectures at the Royal College of Physicians of London on May 2 and 4, on "Some common defects of diet and their pathological signficance."

ON May 11, Professor F. Keeble delivered the first of two lectures at the Royal Institute on "Plant sensitiveness;" and on May 13, Professor O. W. Richardson began a course of two lectures on "The disappearing gap between the X-ray and the ultraviolet spectra." The Friday evening discourse on May 12 was delivered by Dr. H. H. Dale on "The search for specific remedies."

AMONG five busts unveiled in the Hall of Fame for Great Americans at New York University on May 20 was one of Maria Mitchell, the gift of her nephew, William Mitchell Kendall, and the work of Emma S. Brigham. President Henry Noble McCracken, of Vassar College, where Miss Mitchell was professor of astronomy from 1865 to 1888, unveiled the bust.

HENRY MARION HOWE, professor-emeritus of metallurgy in Columbia University, died on May 14 at his home in Bedford Hills, N. Y., in the seventy-fifth year of his age.

DR. JOHN SANDFORD SHEARER, professor of physics at Cornell University since 1910, died on May 18 at the age of sixty-six years.

GEORGE SIMONDS BOULGER, the well known English writer on botany, died on May 4, at the age of fifty-nine years.

SIR ALFRED BRAY KEMPE, president of the London Mathematical Society in 1894, for many years treasurer of the Royal Society, died on April 27, at the age of seventy-three years.

C. L. A. LAVERAN, professor at the Pasteur Institute, Paris, died on May 18, at the age of seventy-seven years. Dr. Laveran, then a French army surgeon serving in Algeria, discovered the parasite of malaria in 1880. He received the Nobel prize for medicine in 1907.

ATHERTON KINSLEY DUNBAR, of Cambridge, fellow for research in cryogenic engineering at Harvard, and William Connell of Cambridge, a carpenter, were instantly killed on May 20, by the explosion of a tank of liquid oxygen in the basement of the Jefferson Physical Laboratory.

SIR CHARLES PARSONS, F.R.S., has conveyed to the trustees of the British Association for the Advancement of Science a gift of £10,000 five per cent. war loan stock, which he has placed unreservedly at the disposal of the council. The London Times writes: "This generous gift comes at an opportune time, as the finances of the association have, like those of other institutions, suffered depletion during the past seven years, and there was a danger that the activities of an association which has rendered notable services to science in the past might suffer restriction. The total grants in aid of research made by the association since its foundation in 1831 exceed £83,000."

THE International Congress of Ophthalmology met in Washington on April 25 and 26. The congress was greeted by Vice-president Coolidge. During the first session, Dr. William H. Wilmer, of Washington, presided. Representatives of many foreign countries attended the meetings. The following officers were elected: *President,* George E. de Schweinitz, Philadelphia, and *secretary,* Luther C. Peter, Philadelphia.

THE Rockefeller Foundation has offered to Indian medical graduates, selected by the scientific board of the Indian Research Fund, five scholarships of $1,000 each, for the purpose of graduate public health work in America.

THE Royal Academy of Belgium has established a prize of 1,000 francs, which will be awarded biennially, under the name of the Prix O. van Ertborn, for the best work on geology.

UNIVERSITY AND EDUCATIONAL NOTES

UNDER the will of the late Mr. Henry Musgrave sums amounting to £57,000 have been bequeathed to Queen's University, Belfast. A Musgrave Research Studentship will be established.

A CONFERENCE of Representatives of the Universities of the United Kingdom was held on May 13 in the Botanical Theater, University College, London. The subjects for discussion were the urgent need for the provision of enlarged opportunities for advanced study and research; the increase of residential accommodation for undergraduate and other students; specialization in certain subjects of study by

certain universities; and the organization of adult education as an integral part of the work of the universities.

DR. DAVID P. BARROWS, president of the University of California, on May 16 presented his resignation as president.

DR. GEORGE P. CUTTEN, president of Acadia University, Nova Scotia, has been elected president of Colgate University at Hamilton, N. Y.

DR. ALAN MARA BATEMAN has been appointed associate professor of economic geology at Yale University, with assignment to the Sheffield Scientific School.

DISCUSSION AND CORRESPONDENCE

THE CYTOLOGY OF VEGETABLE CRYSTALS

THE title of this note involves, especially to those of mechanistic outlook, an apparent contradiction in terms. It is very generally asserted that crystals of calcium oxalate, the commonest type found in plants, are formed by the ordinary processes of crystalization in the fluid of the cell sap, occupying the vacuolated center of the mature vegetable cell. It is the intention of the present preliminary statement to call attention to the fact that this description of the mode of formation of vegetable crystals is in all respects profoundly inaccurate. The commonest type of crystal of calcium oxalate is the compound crystal or druse which prevails from the Ginkgoales to the Angiospermæ. The most favorable object for study is Ginkgo. Longitudinal and transverse sections through the mature tissues as well as through the growing points show the presence of druses in great numbers, and often of large size, particularly in the pith, cortex, and phloem. In spite of the presence of such crystals, sections as thin as five micromillimeters can easily be cut off the tissues. When these are stained and mounted the crystals stand out with particular clearness as occupying practically the entire lumen of the cell.

When measures are taken to remove the calcium oxalate by the use of solvents, the presence of an organic matrix in the crystals becomes obvious, as a residuum maintaining the form of the crystals after the lime compound itself has disappeared. If sections are made in proximity to the growing point, a very interesting situation becomes apparent. The cells in this region are densely filled with protoplasm and those which are to produce crystals are easily recognized from the first. They contain, as do other young cells, a central nucleus and it is obvious in demineralized sections that the crystals are laid down about the nucleus, when the protoplasm of the element is still dense and unvacuolated. From the very beginning the crystals occupy practically the whole lumen of the cell and more or less protoplasm surrounds the nucleus which is the organic center of the druses. The crystals in fact constitute an irregular spiny casing, which surrounds the nucleus and protoplasm. Even in very large and old crystals indications of the presence of a nucleus can frequently be demonstrated by appropriate methods.

Similar observations have been made in the case of crystals of oxalate of lime, so commonly present as a metabolic byproduct in the Dicotyledons. Particularly favorable objects for such studies are the Juglandaceæ, Cactaceæ, Begoniaceæ, Gerianiaceæ, etc. In angiospermous species the nucleus becomes obscured at a very much earlier stage of development of the crystal and not infrequently the latter does not occupy the whole lumen of the cell as in Ginkgo.

Apparently the most interesting fact in the present connection is that compound crystals or druses are not formed in plants by the ordinary routine of crystallization in the watery fluid of the cell sap, as has been universally stated and supposed; but by the action of living protoplasm and under the influence of the nucleus, which is central to the crystal itself. Corresponding to this fact there is only one druse in each cell. A further surprising fact is that the cell-wall in many cases grows in size to accommodate the crystal under the influence of protoplasm contained within the crystal itself. This condition constitutes a very serious problem for those mechanists who attempt to explain all the properties of living beings by the so-called artificial cell and colloidal chemistry. The crystal-containing cells

of the seed-plants do not appear to fit into this conception in even an approximately satisfactory manner.

E. C. JEFFREY

LABORATORIES OF PLANT MORPHOLOGY,
HARVARD UNIVERSITY

RIVER-BANK MOVEMENTS DUE TO THE EARTH'S ROTATION

To THE EDITOR OF SCIENCE: In SCIENCE, March 17, Mr. O. E. Jennings calls attention to a difference between the east and west banks of one of the short streams flowing across the almost flat southern slope of Long Island: "An almost imperceptibly sloping eastern bank and a western bank rising quite steeply." Mr. Jennings says, "This peculiar situation has long been accepted rather generally by geologists and physiographers as due to the *westerly deflection* of streams by the earth's rotation" (italics mine). The statement just quoted is doubtless an accidental slip. The fact is that because of the earth's rotation longitudinal rivers in the northern hemisphere erode their right banks—whether they flow south or north.

In offering another hypothesis for those Long Island banks Mr. Jennings makes the justifiable suggestion that the stream in question—as regards length and velocity—is incompetent for securing through the earth's rotation the effects observed. If it has a narrow channel and carries a small volume of water these items should be added to its other disqualifications. And finally, the latitude of Long Island—less than half the distance from the Equator to the North Pole—is none too favorable for river-bank movement due to the earth's behavior as a heavenly body.

In this connection reference may here be made to the unquestionable evidence of the earth's rotation afforded by the Yenisei. There is probably nowhere else in the world any other stream so favorable for the study of bank movement on a vast scale. This for three reasons: This Siberian river is closely longitudinal; of great size; and so far north that a considerable section of it lies within the Arctic Circle. Dr. Fridtjof Nansen, who has sailed up this river from its mouth to Yeniseisk—a distance of more than a thousand miles—writes of the very pronounced contrast between the east and west banks. "Every one going up the Yenisei must be struck with the remarkable difference between the east and west sides of the river. While the flat land on the east is comparatively high and falls abruptly with a steep bank to the river, a steeply sloping beach and relatively deep water outside, the land on the west is strikingly low. The steep river bank is not high, and the bare sandy beach slopes quite gently to the water, with a shelving bottom far beyond it, so that as a rule it is not easy to approach this shore in a ship or boat." And again, "It is striking how much higher and steeper the east bank is than the west everywhere along here."

Dr. Nansen's observations[1] of this northern river and his discussion of what he saw forms a distinct contribution to the literature of the subject of such river-bank movements as are to be referred to the rotation of the earth.

ELLEN HAYES

WELLESLEY, MASS.

THE DECOMPOSITION OF TUNGSTEN

SIR ERNEST RUTHERFORD, in the statement copied from *Nature* in the April 21 issue of SCIENCE, was in the very difficult position of being "asked to say a few words" in comment on a brief cablegram to the London *Times* which was itself based on an exaggerated Associated Press dispatch to American newspapers concerning the preliminary and oral but as yet unpublished report of Mr. Clarence E. Irion and myself on the apparent decomposition of tungsten at extremely high temperatures. He mentions the need of a complete report before intelligent comment is possible, but proceeds to make three points which are properly conservative and entirely correct but, as will be seen from the complete paper upon its publication in the *Journal of the American Chemical Society,* which are all irrelevant. In view of the publicity given to Sir Ernest's comments in *Nature* and in SCIENCE, however, a few words in reply are needed.

The first point is that the appearance of helium has often been observed in electrical

[1] Nansen, Fridtjof: *Through Siberia, the Land of the Future,* 71, 72, 73, and 157, 158, etc.

discharge tubes during the past ten years but that "it has been generally assumed that this helium has in some way been occluded in the bombarded material." True; we have a list of no less than 37 papers, most of them published in the years 1912 to 1915, engaged in this inconclusive argument. In spite of the application of the best experimental skill no agreement was reached and Rutherford's conclusion is the general one. Yet there are some of the final experiments, particularly those of Collie, which challenge that conclusion and the problem is still one of the most attractive and important of recent times. Certainly it urges conservatism and the most rigorous criticism, yet not one of the papers shows that helium can not be produced and all call for the application of some entirely new method to the same problem. That we have now accomplished.

The second point is that a measure of the energy produced by the atomic decomposition, as predicted by modern theories of atomic structure, would be "a much more definite and much more delicate test of disintegration of the heavy elements into helium than the spectroscope." This is a rare example of the preference for theory over fact, though saved by the use of the word "test" instead of "proof," and the chemist will be slow to accept it. Our work has not gone far enough to permit the measurement of the energy evolved but the latter is certainly not as large as would be expected from the energy liberated in the disintegration of radium. Yet lack of the theoretical energy does not explain away the formation of a cubic centimeter of permanent gas from half a milligram of tungsten wire, though it demands careful scrutiny and, if confirmed, some explanation. Perhaps a lesser energy content accompanies the greater stability of the permanent metals, for even among the radioactive elements the violence of disintegration varies inversely with the stability.

Finally Sir Ernest points out that no helium has been observed in X-ray tubes operating at 100,000 volts, where electron impacts are even more violent than in our experiments. But the quantity of energy impressed on the target is here minute, the tube current being measured in milliamperes or less, whereas it is the essence of our method to introduce as much as a coulomb of electricity into the wire within 1/300,000th of a second, or many millions of times as much in terms of power. We suppose that it is temperature as such, *i. e.*, the high velocity collisions of the atomic nuclei with one another, that effects the atomic decomposition.

We appreciate and welcome the spirit of Rutherford's criticisms. Indeed it is for the purpose of eliciting such criticism and stimulating the laboratory study by other investigators that we are publishing our work in its present preliminary form. The importance of the problem warrants it.

The real question now raised concerns the broadcasting of the results of scientific researches by our publicity agencies. This is an important function and science has suffered from its neglect. Yet our experience shows that it can be overdone, for here is a research heralded as "transmutation" to millions of newspaper readers in at least six countries: it is not transmutation in any proper meaning of that term, it is merely a preliminary report by no means accepted by, or offered to, the scientific world as conclusive, and it must still wait months before it can be properly published in the appropriate scientific journal for the study of those who are competent to appraise it. Meanwhile it is the duty of scientists to urge prudence and conservative judgment, as Sir Ernest Rutherford has done. Our publicity problems are not solved when we have increased the effectiveness of contact with the press.

GERALD L. WENDT

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SCIENTIFIC LITERATURE

RECENT WORK ON SOIL ACIDITY AND PLANT DISTRIBUTION

WHEN three independent investigators, living in different countries, and not knowing of one another's activities, hit upon a similar method of study and reach essentially the same conclusions concerning a set of natural phenomena, it is not unreasonable to infer that a correct understanding of the relations has been reached. For many years it has been customary to regard soil acidity as having no particular bearing on the distribution of native

plants. The Danish ecologist Warming, it is true, distinguished a group of "oxylophytes"—acid place plants—but he had few followers. Coville[1] was successful in cultivating the wild blueberry and other *Ericaceæ* by maintaining the soil in an acid state, but this was looked upon by most botanists as anomalous and exceptional. Because supposed oxylophytes were occasionally found growing with supposed "calcicoles"—lime dwellers—ecologists in general have been inclined to discredit the existence of any definite relation between native plants and soil acidity. During the last few years, however, newly developed methods of interpreting and determining acidity have been applied in several widely separated regions—Sweden,[2] Denmark,[3] the northeastern United States,[4] (and subsequently in India and in England[5]), with the same result in all cases: recognition of the great significance of the acidity of the soil in controlling the growth and distribution of native plants.

The three investigators in question have found independently that the active acidity of a soil can be definitely determined by stirring up a sample with pure water and testing the extract with indicators adapted to show by their color changes the hydrogen-ion concentration. Arrhenius and the writer take their soil samples from the roots of the plants under study, while Olsen takes his at a uniform depth of 10 cm. His samples are therefore representative only of the plants rooting at that depth, and not of the shallower or deeper rooted ones. This renders his data as to some plants uncertain, since most soils show a marked acidity gradient, which may amount to as much as 0.1 p_H unit (equivalent to a factor of 1.25 in specific acidity) per centimeter in depth.

In a recent review, Clements[6] has shown that the production of acidity in bog soils is connected with lack of aeration; but it does not follow that the same is true of upland soils. In the writer's experience the highest acidities in them occur among rock fragments at the summits of mountains, in the dry sands of pine barrens, and in the most loosely packed and thoroughly aerated vegetable débris. This acidity is presumably due chiefly to the development in the soil of such acids as acetic, citric, and lactic, which, like their production for food purposes, is an aerobic oxidation process. In bogs, therefore, there is likely to be an increase of acidity with depth, in dry-land soils a decrease.

Before determining the acidity of a soil, Arrhenius and Olsen allow the water suspension to stand for as much as 24 hours, and then filter. The writer feels that long standing of a soil in contact with excess water may enable reactions, with resulting acidity changes, to take place which would not occur when the soil is in its natural condition, so that fairly prompt testing seems preferable. Moreover, filtration removes fine material which may well contribute to the effect of the soil on a plant, and therefore should be allowed to affect the indicators also. Arrhenius and Olsen make their determinations of the acidity of the modified and purified soil extracts with great precision, using a comparator, the former recommending, however, that a method of determination should always fit the sample. The writer, finding that the variation from one root to another of a single plant, or from one individual to another of the same species, often amounts to 0.5 p_H unit, or a factor of 3 in

1 "Experiments in Blueberry Culture," 1910, *United States Department of Agriculture Bureau of Plant Industry Bulletin No.* 193.

2 Olof Arrhenius: *Œcologische Studien in den Stockholmer Schaeren.* Stockholm, 1920. Review in *Ecology,* II, 223-228, 1921.

3 Carsten Olsen: *Studier over Jordbundens Brintionenkoncentration og dens Betydning for Vegetationen, saerlig for Plantefordelingen i Naturen.* Copenhagen, 1921. Abstract in SCIENCE, LIV, 539-541, 1921. English edition promised.

4 Edgar T. Wherry: A series of papers on ferns, orchids, Ericaceæ, etc. 1916 ——; also: "Soil Acidity and a Field Method for Its Measurement." *Ecology,* I, 160-173, 1920; to be published in collected form in the Appendix to the *Smithsonian Annual Report* for 1920. (In 1922).

5 W. R. G. Atkins: "Relation of the Hydrogen-ion Concentration of the Soil to Plant Distribution." *Nature,* CVIII, 80-81, 1921. Also *Sci. Proc. Royal Dublin Acad.,* XVI, 369-413, 1922.

6 "Aeration and Air-content; the Rôle of Oxygen in Root Activity." *Carnegie Institution of Washington Publ.,* 315, 183 pp., 1921.

specific acidity, developed his method so that it would yield just this degree of precision. These points are mentioned specially because Olsen, in the Danish paper, criticizes the writer's method severely on the basis of "inaccuracy." But if a soil, the acidity of which varies in general by a factor of 3, is sampled at an arbitrary depth and then altered by long soaking and filtration, there is certainly nothing to be gained by making highly precise acidity determinations on the resulting extract. Indeed, both Arrhenius and Olsen, upon assembling the results obtained on given species or associations of plants, also find that there is always a range of at least 0.5 in p_H (a factor of 3 in specific acidity). The fact that all three come to recognize the same range indicates that it is of fundamental significance.

All three investigators find that the soils of native plants in general extend from a specific acidity of a few thousand to a specific alkalinity of about 10. All find that the greatest number of species as well as of individuals occur in soils lying just to the acid side of the neutral point. And, most remarkable of all, it turns out that many individual species of plants have essentially the same soil acidity preferences in Europe as in America, indicating that this is not a question of location, climate, or surroundings, but a physiological feature of the species. For illustration: the lily-of-the-valley, *Convallaria majalis,* grows in Denmark in soils of specific acidity 1000 to 400. Isolated colonies of this plant in the southern Appalachian Mountains have been studied by the writer and found to have specific acidity 500 to 300, practically the same range. Hepatica (*Hepatica triloba* or *Anemone Hepatica*) shows in Denmark preference for soils ranging from neutral (specific alkalinity 1) to specific alkalinity 8. In America a near relative of the European plant thrives best in black leafmold with an average specific alkalinity of 3.

How soil acidity affects plants is a subject requiring further investigation. Olsen's data led him to infer that the action may be direct, but others have found that it is usually indirect. There is evidence both for and against the view that the acidity affects primarily symbiotic organisms, and only indirectly through them the higher plants. Recent American work has indicated that the effect of acidity is produced largely through the agency of aluminium or iron salts, although Olsen is unable to find evidence of their toxicity. But in view of the general agreement of the results of the three independent investigators as above outlined, it can no longer be questioned that soil acidity is of fundamental importance in controlling the distribution of native plants.

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SPECIAL ARTICLES

THE EINSTEIN EQUATIONS FOR THE SOLAR FIELD FROM THE NEWTONIAN POINT OF VIEW

1. About a year ago I determined the law of attraction from the Newtonian point of view of action at a distance which gives the equations of planetary motion obtained in the Einstein theory. Two months ago Professor Birkhoff, of Harvard, told me that he had obtained similar results in his class this year, and suggested that I publish my results. In doing so I am not advocating the rejection of the Einstein point of view which seems to me the correct one, but I am merely indicating a modification in the Newtonian law which will account for the motion of the perihelion of Mercury and the deflection of light rays. It may be also that by means of this formulation of the law it will be possible to solve, with sufficient accuracy, problems which are not readily handled by means of the equations of general relativity.

2. The Schwarzschild form of the linear element of the Einstein field of gravitation of a mass m at rest with respect to the space-time frame of reference is

$$(1)\quad ds^2 = \left(1-\frac{2m}{r}\right)dt^2 - \frac{1}{1-\frac{2m}{r}}dr^2 - r^2(d\theta^2+\sin^2\theta d\varphi^2)$$

where r, θ and φ are the space coordinates as measured by astronomers, and t is the coordinate of time.

If we write

(2) $I =$

$$\sqrt{\left(1-\frac{2m}{r}\right)\dot{t}^2 - \frac{r}{r-2m}\dot{r} - r^2(\dot{\theta}^2 + \sin^2\theta\dot{\varphi}^2)}$$

where dots indicate derivatives with respect to s, the world-lines of particles in the gravitational field are the curves in the 4-space for which the integral

$$\int I ds \tag{3}$$

is stationary. The conditions that (3) be stationary are four differential equations of the second order. From one of them it follows that the path is plane; the coordinates may be chosen so that the equation of the plane is $\theta = 0$. Two of the other equations admit as first integrals

$$\frac{dt}{ds} = \frac{A}{1-\frac{2m}{r}}, \tag{4}$$

$$r^2\frac{d\varphi}{ds} = h, \tag{5}$$

where A and h are constants. It is readily shown that $I = k$, a constant, is a first integral of the four equations. When k is not zero, s can be chosen so that $k = 1$. Then we have $\theta = 0$, (4), (5), and

$$\left(\frac{dr}{ds}\right)^2 + r^2\left(\frac{d\varphi}{ds}\right)^2 = (A^2 - 1) + \frac{2m}{r} + 2m\frac{h^2}{r^3} \tag{6}$$

for the equations of a world-line of a particle in the gravitational field.

When $I = 0$, the integral (3) is stationary and the corresponding world-lines are those of light in accordance with the Einstein theory. Their equations are (4), (5) and

$$\left(\frac{dr}{ds}\right)^2 + r^2\left(\frac{d\varphi}{ds}\right)^2 = A^2 + 2m\frac{h^2}{r^3} \tag{7}$$

Some writers have obtained these equations by solving $I = 0$ for dt and expressing the condition that $\int dt$ be stationary, in accordance with the Fermat principle. The above method was given by Professor Veblen in his lectures, and appears also in Laue, *Die Relativitätstheorie,* Vol. 2, p. 225. Putting $I = 0$ in (2), we see that the units are such that the velocity of light is unity for $r = \infty$, and that it diminishes as the light approaches the sun. If the unit of length is taken as a kilometer, then the unit of time is 1/300,000 of a second.

3. In classical mechanics for a central force of attraction $f(r)$ the equations are

$$r^2\frac{d\varphi}{dt} = h \tag{8}$$

and

$$\left(\frac{dr}{dt}\right)^2 + r^2\left(\frac{d}{dt}\right)^2 + 2\int_\infty^r f(r)dr = E, \tag{9}$$

where h and E are constants. For planetary motion about the sun, whose mass in gravitational units is denoted by m, equation (9) assumes the form

$$\left(\frac{dr}{dt}\right)^2 + r^2\left(\frac{d\varphi}{dt}\right)^2 = -\frac{m}{a} + \frac{2m}{r}, \tag{10}$$

where a is the semi-major axis. For the solar system m/a and m/r are of the order of 10^{-8}, for the units previously defined. Thus if we identify $A^2 - 1$ in (6) with $-m/a$ in (10), $A - 1 = \frac{1}{2}\, 10^{-8}$ approximately. Then from (4), $\frac{dt}{ds} = 1 + 3/2\ 10^{-8}$ approximately, which shows the order of discrepancy so far as the solar system is concerned in interpreting ds and dt as the same in (5), (6), (8) and (9) (*cf.* Eddington, Report, p. 50).

It is well-known that it is the term $2mh^2/r^3$ in (6) which accounts for the motion of the perihelion of Mercury. Comparing (6) and (9), we see that from the point of view of action at a distance this is accounted for if we take

$$f(r) = m\left(\frac{1}{r^2} + \frac{3h^2}{r^4}\right). \tag{11}$$

From the preceding remarks it follows that if we put

$$\omega = \frac{d\varphi}{ds} \tag{12}$$

then ω may be interpreted as the angular velocity of the planet about the sun. Then from (5), (11) and (12) we have that

(13) The attraction $=$

$$m\left(\frac{1}{r^2} + 3\omega^2\right) = \frac{m}{r^2}(1 + 3v^2),$$

where v is the component of the velocity perpendicular to the radius vector.

We have remarked in the preceding that the velocity of light at ∞ is equal to 1 in the units

chosen. If we denote it by c in any system of units, we may formulate the law as follows:

Two bodies attract one another inversely as the square of their distance and directly as the product of their masses and $(1 + 3v^2/c^2)$, *where v is the component of their relative velocity perpendicular to the line joining the bodies.*

The form (1) is obtained from the Einstein theory on the hypothesis that the planet is small in comparison with the sun. It may be that the above law applies only to this case. However, it may be that the law would work if the bodies were approximately of the same mass. As formulated the law enables one to set up the differential equations of n bodies in a manner analogous to the classical theory. It would be interesting to know whether known discrepancies in the motion of the moon would be overcome by the use of this law.

Although the term $3v^2/c^2$ produces an observable effect only in the case of Mercury, it may produce a significant effect in molecular motion.

4. When in like manner equation (7) is compared with (9) we find that for a ray of light the attraction is

$$3m\omega^2 \tag{14}$$

where ω may be interpreted as the angular velocity of the light about the sun. Thus it is the term $3m\omega^2$ in (13) which accounts for the deflection of light, and the term m/r^2 does not enter. Einstein and his followers have calculated the deviation of light by noting that the velocity changes in a manner analogous to that of a refracting medium, and by applying Huygen's principle. Since the same term appears in the attraction of a planet, it may very well be that the sun affects the medium through which both the light and planets pass, and that the difference between Newton's law and (13) is due to this situation. From this point of view one would expect that the law would not be accurate for two or more bodies of relatively the same mass, but it may lead to a sufficiently close approximation.[1]

LUTHER PFAHLER EISENHART

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[1] I have just found that A. V. Bäcklund in the *Arkiv för Matematik, Astronomioch Fysik*, Vols. 14 and 15 (just received) has made an extensive study of the relation between classical dynamics and the Einstein theory of gravitation. In the course of his three articles he obtains equation (11) and one similar to (13).

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SECTION F—ZOOLOGICAL SCIENCES AND ASSOCIATED SOCIETIES

At the Toronto meeting of the American Association for the Advancement of Science, December 27-31, 1921, Section F (Zoology) offered no separate program, but met jointly with the American Society of Zoologists. The program was arranged by the latter society.

Six joint sessions were held, the program including 101 titles distributed by subject as follows: embryology, 4; cytology, 8; comparative anatomy, 7; evolution and genetics, 24; ecology and zoogeography (with the Ecological Society of America), 13; general zoology, 2; protozoology, 2; parasitology, 22; comparative and general physiology, 17; unclassified, 2.

The session of Friday afternoon, December 30, was devoted to a symposium on orthogenesis. A biologists' smoker was held Wednesday evening, December 28, and the zoologists' dinner Friday evening, December 30.

The business meeting of Section F took place at the morning session on December 29, with Vice-president Kofoid acting as chairman. M. M. Metcalf is vice-president for Section F for 1922. J. A. Detlefsen was elected a member of the section committee for four years in place of the retiring member, A. M. Reese.

F. R. Lillie presented the following resolutions drawn up by a conference of representatives of the biological societies in regard to a proposed federation of biological societies:

RESOLVED: 1. That it is the sense of this conference that an inter-society conference should be called to study and report upon the feasibility of federation of the biological societies and to develop plans for the said federation.

2. That for the purpose of effecting such an organization, each society, and Sections F and G of the American Association for the Advancement of Science, be requested to designate its president and secretary as members of an inter-

society council which shall be authorized (1) to deal with all matters of common interest, such as pooling of programs, that are consistent with the existing regulations of the constituent societies, and (2) to draw up proposals for a constitution and by-laws of a federation of the societies in question, and to present them for action at the next annual meeting.

The Section voted that the resolutions of the conference be adopted.

Independent programs were arranged by the following societies affiliated with Section F—The Entomological Society of America, The American Association of Economic Entomologists; and by the following societies affiliated with Sections F and G jointly—The American Society of Naturalists, The Ecological Society of America, The American Microscopical Society (business meeting only), The American Nature-Study Society.

HERBERT W. RAND,
Secretary, Section F

SECTION G—BOTANICAL SCIENCES AND ASSOCIATED SOCIETIES

SECTION G held its session on Wednesday afternoon, December 28, 1921, in conjunction with the Botanical Society of America and the American Phytopathological Society. There was a large attendance at this meeting, and the symposium, though involving several papers, was not unduly long. Professor Rodney H. True, retiring vice-president for Section G, delivered his address on "The physiological significance of calcium for higher green plants," which has been published in SCIENCE, Vol. LV, p. 1, January 6, 1922. The vice-presidential address was followed by a symposium on "The Species Concept," at which the following papers were read: (1) "From the viewpoint of the systematist," Charles F. Millspaugh; (2) "From the viewpoint of the geneticist," George H. Shull; (3) "From the viewpoint of the morphologist," R. A. Harper; (4) "From the viewpoint of the bacteriologist and physiologist," Guilford B. Reed; (5) "From the viewpoint of the pathologist," E. C. Stakman. The writers of these papers cooperated splendidly, both in division of subject matter and in time of presentation. The results seem to confirm the expressed belief of many botanists that a symposium of general interest, making appeal to workers in all the principal fields, is well worth while.

At the business session of Section G, John T. Buchholz, of the University of Arkansas, was elected to be a member of the section committee, his term of office to end January 1, 1926. Professor F. E. Lloyd of McGill University, was selected as vice-president for Section G for 1922.

Botanical Society of America.—This society held sessions beginning Wednesday morning, December 28, 1921, and continuing through Friday. On Thursday afternoon the Mycological Section held a joint session with the American Phytopathological Society, and on Thursday afternoon the Physiological Section met in conjunction with the American Society for Horticultural Science and the Ecological Society of America. At the sessions of the Botanical Society of America, eighty-seven scientific contributions were read. The dinner for all botanists was held on Friday evening. After the dinner Dr. Marshall Howe read "A Communication from the Retiring Vice-president," Dr. N. L. Britton.

American Phytopathological Society.—Sessions of this society were begun on Wednesday morning, December 28, and continued until Saturday morning. At the business sessions of this society the following officers were elected: *President,* E. C. Stakman, University of Minnesota, St. Paul, Minn.; *vice-president,* N. J. Giddings, University of West Virginia, Morgantown, W. Va.; *secretary and treasurer,* G. R. Lyman, Bureau of Plant Industry, Washington, D. C. One hundred and seven scientific contributions were read during the sessions. The Phytopathologists' dinner was held on Thursday evening, the dinner being followed by a discussion of important topics, and a short business session.

Board of Control of Botanical Abstracts.—Business meetings of the Board of Control were held on Tuesday, Wednesday and Thursday. During these meetings various matters were given attention including the election of editors and various considerations in connection with the publication, financial support, and circulation of *Botanical Abstracts.*

The Toronto meeting, from the viewpoint of the botanists, was a very successful gathering, and the attendance of plant workers was greater than had been anticipated.

ROBERT B. WYLIE,
Secretary

SECTION I—PSYCHOLOGY

THE meeting of Section I (Psychology) at Toronto was a very successful one. Although the affiliated society was meeting elsewhere, a considerable number of American psychologists attended the sessions, and to these were added several Canadian psychologists and a good many professional men and women who are interested in psychology from the point of view of its practical applications to education, business, criminology and related fields. The program was enriched by contributions from a number of men who represented these interests. The discussion of the papers was lively and in some sessions had to be limited for lack of time. The attendance at the meetings averaged about 25 and reached 125 at one session.

As is usual, there was at the Toronto meeting an intimate relation between the sessions of Sections I and Q (Education). Sessions were held conveniently in the same building, and two were joint sessions. The papers in these sessions dealt with mental tests or with psychological studies in education. There was apparent in the discussions of mental tests a disposition to examine somewhat more critically the conclusions to be drawn from the results of mental tests than has prevailed in the past. Of the other papers special mention may be made of one by Professor Thorndike in which he distinguished two types of equation—the equation for solution and the equation which expresses relationship—and advised that special care be taken to avoid confusion between the two.

The first session was devoted to general papers. Professor Dale discussed the place of psychology in university curricula, emphasizing the need of giving it reality by relating it to the practical problems of life. Professors Brett and Pillsbury discussed a number of the important issues on which modern psychologists differ, and Professor Weiss discussed variability in behavior as a basis of social interaction.

One morning session was devoted to applied psychology. The problems in this field were discussed from the point of view of employment relations, of job analysis, and of dealing with the handicapped in occupation, by Mr. George W. Allen, Professor E. K. Strong, Jr., and Mr. Norman L. Burnett, respectively. Dr. Alfred E. Lavell, chief parole officer of Ontario, described the beneficial effects of supervised employment upon paroled prisoners.

The last session opened with two general papers on mental tests and their significance. Professor William D. Tait argued that education should be highly selective and adapted to intellectual capacity. Dr. R. M. Yerkes emphasized the need of other types of mental examination in addition to intelligence tests. The results of psychiatric and intellectual examination of Illinois prisoners were presented by Dr. Herman M. Adler. In agreement with the results of an Ohio study, his examination showed that prisoners are not a select group intellectually. He indicated, however, that they do exhibit anomalies of behavior. Psychiatry in the public schools was discussed by Dr. Eric K. Clarke. A study of the divergence between the color preferences of Indians and whites was reported by Professor T. R. Garth.

The address of the retiring vice-president, Professor E. K. Strong, Jr., dealt with the problem of propaganda. He discussed and illustrated propaganda in business, politics, and social reform (or pseudo-reform), and raised the question whether it is possible to control it or neutralize its effects. Control he recognized as very difficult, but suggested that it might be necessary to modify the legal theory of refraining from interference until propaganda could be shown to issue in overt acts. The essential nature of propaganda is appeal to the emotions, and this makes control useless unless it takes effect when the general emotional foundation for overt action is being laid. The emotional character of propaganda also makes difficult its control through merely intellectual illumination.

A joint dinner and smoker with Section Q

was held on Wednesday evening at which short speeches were made by Dr. R. M. Yerkes and Dr. H. Addington Bruce.

The vice-president of the section for next year's meeting, at Boston, is Professor Raymond Dodge, and the new section committeeman is Dr. Yerkes.

FRANK N. FREEMAN
Secretary, Section I

SECTION O—AGRICULTURE AND ASSOCIATED SOCIETIES

SECTION O met on Wednesday afternoon, December 28, 1921, with six associated societies. The program of the meeting consisted of a symposium on "The Cooperation of Canada and the United States in the Field of Agriculture." Dr. E. W. Allen, of Washington, delivered the retiring vice-presidential address on "The Method of Science in Agriculture," calling attention to the importance of the utilization of the most accurate scientific methods in agricultural investigations, pointing out ways in which certain lines of study now under way may be made more comprehensive and urging that attention be given constantly to the improvement of methods and that the interpretaion of all results be based more directly upon the methods employed in the work.

Following the vice-presidential address, the following papers were read:

Marketing Conditions in Canada: ARCHIE LEITCH, Ontario Agricultural College, Guelph, Canada.

Organization for research in the United States: L. R. JONES, chairman, Division of Biology and Agriculture, National Research Council.

Cooperation in research: J. H. GRISDALE, deputy minister of agriculture, Ottawa, Canada.

Some economic aspects of the wheat situation: (Illustrated with lantern slides): CARLETON R. Ball, Bureau of Plant Industry, Washington, D. C.

History and development of the Canadian Society of Technical Agronomists: F. H. GRINDLEY, Gardenvale, P. Q., Canada.

The attendance at the meeting was very gratifying, over 50 persons being present. The addresses which were given were extremely interesting, and each was followed by considerable discussion. Particular interest was evidenced in the suggestions regarding organized research and greater cooperation between the United States and Canada in the development of research activities.

At the business session of the section, R. W. Thatcher, of the New York Agricultural Experiment Station, Geneva, N. Y., was elected vice-president, and E. W Allen, of the United States Department of Agriculture, Washington, D. C., was elected a member of the section committee, his term of office to end January 1, 1926.

At the conclusion of the meeting a dinner was held at Queen's Hall; a large number of the members of the section were in attendance. This proved to be a most enjoyable occasion, and it is hoped that a dinner for Section O and all associated societies may be arranged at subsequent meetings of the association.

The meeting and dinner of Section O were highly successful in every way, and all those in attendance were enthusiastically in favor of having similar arrangements made for later meetings. The associated societies all have their programs; it is conceded that Section O should give a more general, somewhat introductory, program and one which will be of interest to all agricultural organizations. This feature of the program at Toronto was particularly successful, the dinner being an innovation which everyone felt had added materially to the success of the meeting.

The American Society of Agronomy.—This society held a meeting on Thursday, December 29, 1921, at which a general program of agronomic interest was prepared. Ten scientific contributions were presented; each was followed by considerable discussion. About 40 agronomists from Canada and the United States were in attendance, and the meeting was a most successful one in every way. Matters of general interest to both crops and soils men were discussed, and the exchange of ideas between the Canadian and United States investigators was particularly valuable. Since this was not the annual meeting of the society, no business was transacted. Resolutions were adopted, however, urging the continuation of the publication of the *Experiment Station Record* and the *Journal of Agricultural Re-*

search, by the United States Department of Agriculture.

The American Society for Horticultural Science.—This society held sessions beginning on Wednesday and continuing through Friday. The scientific contributions, of which there were 31, were given in groups under the heading of "Breeding," "Fruit Setting," "Nutrition," "Growth Studies," and "Extension."

On Thursday afternoon a joint session with the Botanical Society and the Ecological Society consisted of a symposium on "Frost Resistance, Hardiness, and Winter Killing of Plants." At the symposium the following papers were read:

Geographical distribution of low temperature conditions: Forrest Shreve, Tucson, Ariz.

Observation on hardiness in Canada: W. T. Macoun, Ottawa, Canada.

Relation of water retaining capacity to hardiness: J. T. Rosa, Jr., Columbia.

Effect of low temperature storage and freezing on fruits and vegetables: L. E. Hawkins, Washington, D. C.

A colloidal chemical basis for resistance to low temperatures: R. Newton, St. Paul, Minn.

Physiological and chemical studies of fruits in storage: J. R. Magness, Canton, Pa.

Hardiness from the horticultural point of view: M. J. Dorsey, Morgantown, W. Va.

There was a large attendance of horticulturists, and much interest was evidenced in the program at all sessions.

On Thursday the annual dinner of the society was held; this proved a very enjoyable occasion, the discussion following the dinner centering around the proposed horticultural journal.

The Society of American Foresters.—A joint meeting of this society was held with the Canadian Society of Forest Engineers on Tuesday and Wednesday. Thirty-three scientific papers were presented at the various sessions, being grouped under: "Fire Protection and Forest Administration," "Silviculture and Forest Pathology," "Utilization and Wood Technology," "Regulation and Management," and "Forest Botany."

The members of the society were entertained at dinner on Tuesday evening at the Hart House by the Canadian Society. After the dinner, the retiring presidential address was given by Frederick E. Olmsted, on "Professional Ethics." Other informal addresses were made on this occasion. There was a large attendance of foresters. The program was very much appreciated, and the various papers aroused considerable discusson.

Association of Official Seed Analysts.—This society met on Wednesday, Thursday and Friday, with a general program at each session. Thirty scientific papers were presented; there was considerable discussion of the various papers. One of the most outstanding features of the meeting was the report of the president, Mr. George H. Clark, on the Copenhagen Conference.

Geneticists Interested in Agriculture.—This society met on Tuesday with a program consisting of a symposium on "The Genetics Curriculum in the College of Agriculture." Four papers were presented, and each was followed by a general discussion among the members present. About forty were in attendance, and great interest was evidenced in the methods which are being followed at various institutions in the teaching of genetics to agricultural students.

Potato Association of America.—This society met on Friday with a program consisting of reports of standing committees of the society on "Varietal Nomenclature and Testing," "Market Standards and Marketing," "Transportation," "Educational," "Judging Standards," "Relation of Varietal Type to Yield," "Investigations on Immature Seed Potatoes," and "Seed Potato Certification Standards."

There was a large attendance at the meeting of the society, and keen interest was evidenced in the papers presented. An exhibit of seed potatoes proved of particular interest, many of the states being represented, as well as the Province of Ontario. At the business session of the society, the following officers were elected: *President,* J. G. Milward, Madison, Wis.; *vice-president,* C. A. Zavitz, Guelph, Ont.; *secretary-treasurer,* Wm. Stuart, Washington, D. C.

P. E. Brown,
Secretary, Section O, Agriculture